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of Japan. The translation is interesting, not only as settling a fact of importance, but as indicating the alertness of the Japanese mind in prosecuting scientific inquiry. The article is from the *Zigi-shimpo* of November 5, 1902.

G. FREDERICK WRIGHT.

OBERLIN COLLEGE,
February 4, 1903.

Nobody has ever found a trace of a glacier in our country, and in fact it has often been doubted that one existed in Japan. Professor Milne, of England, who once held a chair in the Imperial University of Tokyo, even went so far as to deny its existence in Japan. It is, therefore, interesting to learn that Professor Yamazaki, of the Higher Normal School of Tokyo, recently found a trace of one on a mountain side in Shinano. When he was interviewed, he gave the following accounts of his discovery:

"The fact that America and Europe were once covered with ice is now beyond dispute; and recently we heard that traces of a glacier were found in Australia; and I have always held a theory that Japan is qualified to have a glacier, for the following reasons.

"1. There are several mountains as high as and above 3,000 meters.

"2. Many of them are covered with perpetual snow.

"3. The climate, being 'oceanic,' the amount of rain and snow is greater here than it is in Europe.

"4. In America, I found that the glacier region comes as far south as the lowest extremity of 37° 60' N. L. Now, Tokyo being on 35° 41' N. L., the middle part of the island along the coast of the Japan Sea corresponds with the glacier region in America.

"I had held this as a mere theory until last August, when I actually found traces of a glacier in one of the northwestern mountains.

"Last August, as a Committee on the 'Prevention of the Earthquake Disasters,' I climbed up a volcano, located on the boundary of three countries, Shinano, Yetchu and Yechigo; and when making investigations in Hida range, I actually fell upon a trace of a glacier on the side of Shira-Umaga-Take. This place, which is 2,900 meters above the

sea, forms a sort of valley, extending, say, for about 200 yards, and the layer of snow is about 20 yards deep. The sides of the valley are composed of slate-rock and sand. Pebbles and pieces of rock found on the mountain are unlike those which we generally find in ordinary mountains—smooth and striated. The rocks along the snow line are marked with grooves and the rock-floor is marked by the grinding work done by a glacier. In a still lower part of the valley, further down, I found stones and rocks traversed in every direction. I have found sufficient evidence to form a belief that here we have the proof of the existence of a glacier in Japan. The erosion is effected by the ice pressing against the sides, as it crept along, taking sand and stones which fell from the sides. If we should follow the range up to the province of Hida, I believe, we should find more valuable proof of the existence of glaciers. At any rate, we certainly have sufficient proof now for clearing the doubt of the existence of glaciers in this country.

"It is a strange coincidence, but a few days later, Professor Yabe found a zone of vegetation like that of the Alps and Chishima, in the very same place."—As he was interviewed by the editor of *Zigi-shimpo*.

November 5, 1902.

SHORTER ARTICLES.

TYPES OF PRE-LINNEAN GENERA.

INSTABILITY in the application of generic names is undoubtedly the most serious remaining deficiency of our current systems of biological taxonomy. To secure stability of specific names a definite rule of priority was sufficient because it had occurred to nobody to deny that the specimen first named and described should constitute the type of the species and determine the application of the specific name. With genera also stability is not to be secured merely by observing priority of dates, since it is necessary that writers agree upon the application of a name as well as upon its age; but by treating each generic name as inseparably attached to a single species as its nomenclatorial type, the law of

priority is rendered as effective with genera as with species.

Many systematists have been content to follow in a general way the varying nomenclatures of their predecessors, while others who have appreciated the importance of uniform procedure have experimented with what has been called the method of residues or elimination, under which a generic name is inherited by the last species left in the genus after all possible segregations have been made. This plan is defective in theory, very difficult of application, and does not bring about uniformity in practice, because different systematists commonly differ as to which species were rightly removed from the genus, and consequently as to which in reality remained to the last. Those who look upon stability as the prime requisite of a system of formal nomenclature are accordingly beginning to abandon elimination in favor of the selection of types by a definite method of priority, but progress in this desirable direction is greatly retarded by the fact that the rule which recognizes 1753 as the beginning of binomial nomenclature would have the unforeseen and very undesirable result of associating many old and well known generic names with species for which they are not currently used, that is, if it were not possible to find a means of avoiding the difficulty.

It seems certain that the consistent application of any method will result in many changes of names, since even in instances where genera were established for single species their names have frequently been slipped along to groups of plants quite unknown to the original authors. Rather than run the risk of having to use old names in new and unexpected places, some would give over the attempt at securing stability. But to those who perceive that taxonomic study is largely a waste of time unless it can be carried on under rules which guarantee uniform nomenclatorial results, no changes essential to the application of such an improved method will seem intolerable or ridiculous, though to make unnecessary changes, even to carry into effect a good rule, would be foolish. Because the guinea-pig would become *Mus* and the

giraffe *Cervus* is not a reason why we should not, in general, treat the first species as the type of its genus; it is simply a reason why we should find, if possible, a means by which an undesirable incident may be avoided without losing the important advantage of a method which all can apply with uniform results. The plan of treating the generic names adopted from pre-binomial writers as a special case should not be opposed even as an exception to the general rule, since with these we are not dealing with the normal method of establishing genera, but are attempting to arrange as smooth a connection as possible between two periods of botanical history. It is true that we are not following the intent of eighteenth-century authors, since we now think of generic names as attached to species rather than to definitions or concepts, but this should not make us unwilling to preserve as many of the older names as possible, nor careless in applying them as nearly as possible in accordance with historical usage.

Many of the older generic names which would be transferred by taking either the first or the last Linnæan species as type may be kept in their customary places by selecting as types species having such names as *officinalis*, *utilis*, *communis*, *vulgaris*, *verus*, *typicus* or others indicative of botanical prominence or popular interest. A rule containing a list of such names would facilitate the selection of types and would be open to no charge of indefiniteness.

Another practical suggestion is that instead of taking as types the first species placed by Linnæus under names adopted from pre-binomial writers, we take the species under which Linnæus gives the oldest citation under the same generic name. This would place the question of types on a definite basis of chronology, and opens no doors to individual differences of opinion. It would require considerable bibliographic labor to locate the oldest citation under some of the larger genera, though this task is much more simple and direct than the method of elimination. The utility of such a rule for the purpose for which it was intended will depend, however, on whether Linnæus followed a method of

citation of such consistent historical thoroughness that his oldest references generally fall on the oldest and best known species of each genus. The indications are that he did not, but often gave citations to old books under relatively little known species which were not well represented in the writings of his more immediate predecessors.

If this should prove to be the case we would save names as well as labor by beginning our historical investigations with Tournefort, who was generally careful to place the most common and best known species at the head of his list. Moreover, such a limitation would enable us to frame a rule of much more direct and easy application, for instead of being obliged to compare the chronology of the Linnæan species of a genus we could simply look for its type where the name first appeared in Tournefort's 'Institutiones' or some later work. If it were found that this species had been included as a binomial in the 'Species Plantarum,' or wherever the generic name was first used by a binomial author, this would constitute the adoption of the pre-Linnæan genus, and its type species would have been determined historically, but still in an entirely definite and invariable manner. Such a rule might read something as follows:

A genus is treated as having been adopted from Tournefort or a later nonbinomial writer when its type species was included under the first binomial use of the name.

This rule would have the further distinct advantage that generic names borrowed by Linnæus from older literature, but applied to new groups of plants, would not be disturbed, since their pre-Linnæan types would not be found under the Linnæan use of the name, which would then be treated as though it had originated with Linnæus or any later botanist. Generic names, like those of species, would have a definite order of priority under the binomial system of nomenclature. All the real advantages of beginning generic nomenclature with Tournefort would be secured, without the folly of resurrecting the many generic names which did not come into use under the binomial system, but have rested in oblivion for a century and a half.

It has seemed desirable to call attention to this alternative suggestion at the present time because its merits can be most readily and satisfactorily investigated while botanists are testing the recently proposed rule to select types of Linnæan genera on the basis of the oldest reference.

O. F. Cook.

WASHINGTON,

February 3, 1903.

A GRANT FROM THE CARNEGIE INSTITUTION FOR
PALEOBOTANY.

THE executive committee of the Carnegie Institution has approved a grant of \$1,500 to G. R. Wieland, of the Yale University Museum, for the continuation during the year 1903 of his researches on the structure of the living and fossil cycads. In connection with this announcement the following brief statement is appended concerning the extent and progress of cycad investigation:

The cycadaceous nature of certain silicified stems with leaves and fruits unknown, from the English Wealden, was recognized as early as 1825. Nearly fifty years later Carruthers studied a similar remarkably preserved trunk from the Lower Greensand of the Isle of Wight, in which he discovered between the old leaf bases, which were thickly covered by ramental hairs like those of ferns, wonderfully preserved and nearly mature ovulate strobili of entirely different structure from those of any cycads known.

About this same time Williamson described certain cycadean leaf imprints as found associated with trunks and various casts of fruits of puzzling character from the cliffs of Hawkser and Runswick on the south coast of England. Nevertheless, these plants remained one of the most interesting of all paleobotanical riddles for the next thirty years, our knowledge of them being confined to their trunk structure and the ovulate strobilus, though it should be mentioned that Capellini and Solms found pollen grains in an imperfectly preserved fruit borne on a trunk found at the ancient Etruscan Necropolis of Marzabotto, thus showing that whatever the character of the male fructification, it must have been borne laterally like the seed-bearing cones.